

# Child Penalties Across Countries: Evidence and Explanations\*

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## Abstract

This paper provides evidence on child penalties in female and male earnings in different countries. The estimates are based on event studies around the birth of the first child, using the specification proposed by [Kleven \*et al.\* \(2018\)](#). The analysis reveals some striking similarities in the qualitative effects of children across countries, but also sharp differences in the magnitude of the effects. We discuss the potential role of family policies (parental leave and child care provision) and gender norms in explaining the observed differences.

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# 1 Introduction

Despite considerable gender convergence over time, substantial gender inequality persists in all countries. Recent work highlights the importance of parenthood for the persistence of gender inequality in labor market outcomes. [Kleven \*et al.\* \(2018\)](#) estimate the impact of children on the labor market outcomes of women relative to men — child penalties — in Denmark. They show that the long-run child penalty in earnings is about 20% and that this can explain most of the remaining gender inequality. Research on other countries suggests that this is a pervasive phenomenon.<sup>1</sup>

The main contribution of this paper is to estimate child penalties in different countries using the same empirical approach, specification and sample selection. We consider six countries that span a wide range of policies and norms: two Scandinavian countries (Denmark and Sweden), two German-speaking countries (Germany and Austria), and two English-speaking countries (United Kingdom and United States). The analysis reveals some striking similarities in the qualitative effects of children, but also some sharp differences in the magnitude of the effects. We end the paper with a discussion of likely explanations for these differences.

## 2 Child Penalties: Methodology

We estimate the impact of children on the labor market trajectories of mothers and fathers using event studies around the birth of the first child. This approach requires high-quality panel data with information on labor market outcomes and children. For the Scandinavian countries and Austria, we leverage the availability of administrative registers for the full population over many years. For the other countries, we use surveys with sufficiently large sample sizes and long time series: the GSOEP in Germany, the PSID in the US, and the BHPS in the UK. We consider individuals who have their first child between the ages of 20 and 45, and who are observed in each year between 5 years before and 10 years after child birth.<sup>2</sup>

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<sup>1</sup>See e.g., [Angelov \*et al.\* \(2016\)](#) and [Kuziemko \*et al.\* \(2018\)](#).

<sup>2</sup>For the PSID and BHPS, we relax the latter restriction in order to increase sample size, focusing on individuals who are observed at least 8 times over the event study window as well as at least once before birth and once after birth.

We adopt the event study specification proposed by [Kleven \*et al.\* \(2018\)](#). For each parent in the data, event time  $t$  is indexed relative to the year of the first child birth. Denoting by  $Y_{ist}^g$  the outcome for individual  $i$  of gender  $g$  in year  $s$  and at event time  $t$ , we run the following regression separately for men and women

$$Y_{ist}^g = \sum_{j \neq -1} \alpha_j^g \cdot \mathbf{I}[j = t] + \sum_k \beta_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \gamma_y^g \cdot \mathbf{I}[y = s] + \nu_{ist}^g. \quad (1)$$

The first term on the right-hand side includes event time dummies, the second term includes age dummies (to control for life-cycle trends), and the third term includes year dummies (to control for time trends). We omit the event time dummy at  $t = -1$ , implying that the event time coefficients measure the impact of children relative to the year just before the first child birth. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child. [Kleven \*et al.\* \(2018\)](#) lay out the identification assumptions underlying this approach, compare its results to alternative approaches in the literature, and provide evidence of its ability to identify the causal effect of parenthood.

Our main outcome variable is gross labor earnings, excluding taxes or transfers, specified in levels.<sup>3</sup> We convert the estimated level effects into percentages by calculating  $P_t^g \equiv \hat{\alpha}_t^g / \mathbb{E}[\tilde{Y}_{ist}^g | t]$  where  $\tilde{Y}_{ist}^g$  is the predicted outcome when omitting the contribution of the event dummies.<sup>4</sup> Having estimated the impacts of children on women and men separately, we define the *child penalty* at event time  $t$  as  $P_t \equiv \frac{\hat{\alpha}_t^m - \hat{\alpha}_t^w}{\mathbb{E}[\tilde{Y}_{ist}^w | t]}$ . This measures the percentage by which women are falling behind men due to children.

### 3 Child Penalties: Results

Figures 1-3 show the effects of parenthood on earnings across the different countries. The results confirm that the existence of large child penalties is a pervasive phenomenon. In each country,

<sup>3</sup>We specify equation (1) in levels rather than in logs to be able to keep the zeros in the data (due to non-participation). In the online appendix, we present separate results on the extensive margin impacts of children.

<sup>4</sup>To be precise, we define  $\tilde{Y}_{ist}^g \equiv \sum_k \hat{\beta}_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \hat{\gamma}_y^g \cdot \mathbf{I}[y = s]$ . Hence,  $P_t^g$  captures the year- $t$  effect of children as a percentage of the counterfactual outcome absent children.

the earnings of men and women evolve similarly before parenthood — after adjusting for lifecycle and time trends — but diverge sharply after parenthood. Women experience a large, immediate and persistent drop in earnings after the birth of their first child, while men are essentially unaffected. Ten years after child birth, women have not recovered and at this point the series have plateaued.

Despite these similarities, the graphs also reveal some striking differences. First, the size of the long-run child penalty (defined as the average penalty from event time 5 to 10) differs substantially across countries. The Scandinavian countries feature long-run penalties of 21-26%, the English-speaking countries feature penalties of 31-44%, while the German-speaking countries feature penalties as high as 51-61%. Second, the short-run dynamics of child penalties show some interesting differences. For example, while the Scandinavian countries are roughly similar in the long run, the short-run child penalty is about twice as large in Sweden as it is in Denmark. Swedish mothers catch up with Danish mothers over time such that their child penalty is only slightly larger after 10 years.<sup>5</sup> Sweden is also the only country where child birth is associated with a small short-run effect on men, although there are no long-run consequences. When considering the US and the UK, we see that these countries feature less dramatic short-run effects, but that the effects are growing over time.

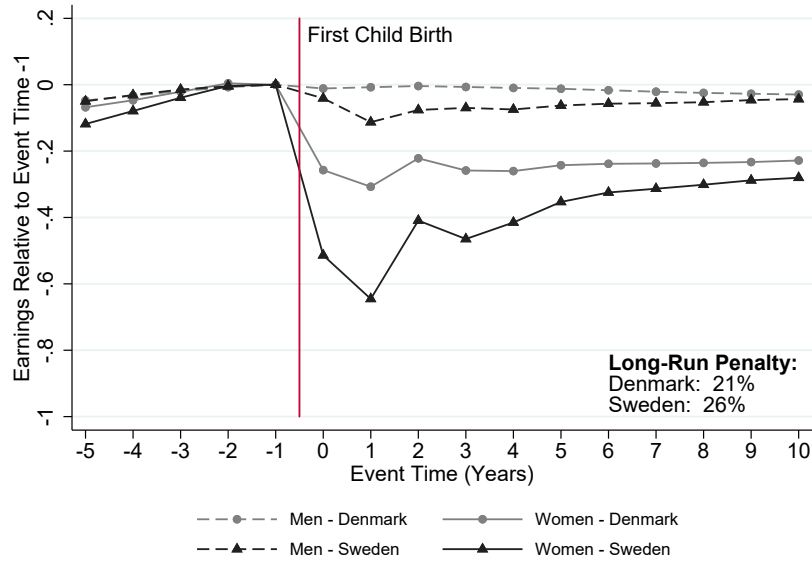
In general, the earnings penalties can come from three margins: the extensive margin of labor supply (employment), the intensive margin of labor supply (hours worked), and the wage rate. In the online appendix, we provide evidence on child penalties along the extensive margin. While parenthood reduces female employment everywhere, the importance of this margin varies across countries. In the Scandinavian and Germanic countries, the extensive margin effects are significantly smaller than the earnings effects, implying that a substantial fraction of the earnings penalty is driven by the intensive margin and wage rate effects. In the US and the UK, the employment penalty is much closer in magnitude to the earnings penalty, suggesting that the extensive margin is a key driver of penalties in those countries.<sup>6</sup>

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<sup>5</sup>Angelov *et al.* (2016) estimate child penalties for Sweden using a different event-study specification. An advantage of implementing the same specification across countries is that it allows for direct comparisons. The fact that Denmark and Sweden are so different is *a priori* surprising. We note that our earnings measure in general includes any (non-mandated) parental leave benefits paid by the employer, implying that cross-country comparisons partly reflect variation in such benefits. While employer-provided parental leave benefits do tend to be higher in Denmark than in Sweden, this is likely to have a modest impact on the relative child penalties for two reasons. One is that such employer-provided benefits were relatively small during the period we study (in Denmark we are considering first child births between 1985-2003), and the other is that those benefits are provided only during event times 0 and 1.

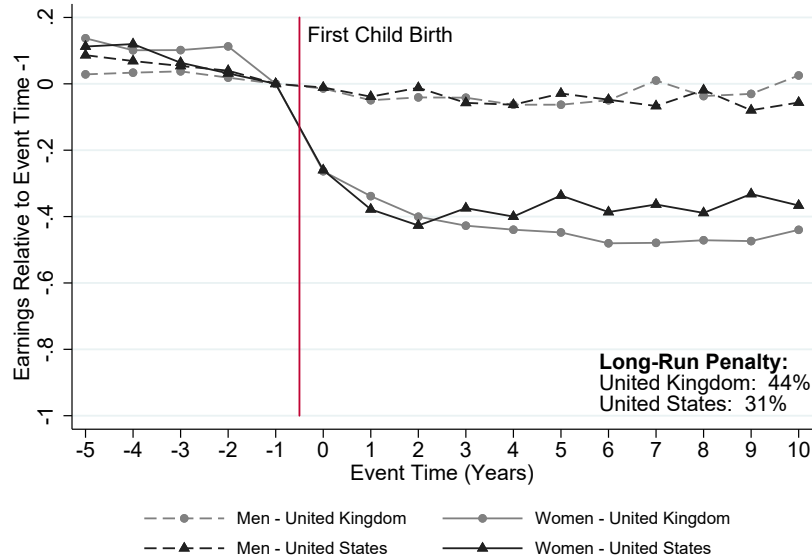
<sup>6</sup>Since we do not condition our samples on having only one child, the long-run child penalties will include the

Figure 1: Child Penalties in Earnings in Scandinavian Countries



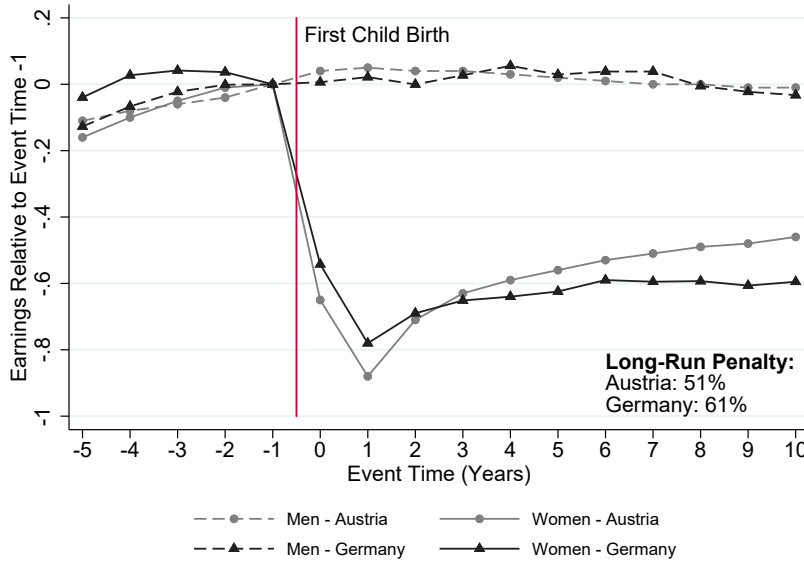
Notes: The figure shows percentage effects of parenthood on earnings across event time  $t$  for each gender  $g$ , i.e.  $P_t^g$  defined above. The figure also displays long-run child penalties, defined as the average penalty  $P_t$  from event time 5 to 10. Earnings are unconditional on employment status and the effects therefore include both the extensive and intensive margins.

Figure 2: Child Penalties in Earnings in English-Speaking Countries



Notes: See the notes to Figure 1.

Figure 3: Child Penalties in Earnings in German-Speaking Countries



Notes: See the notes to Figure 1.

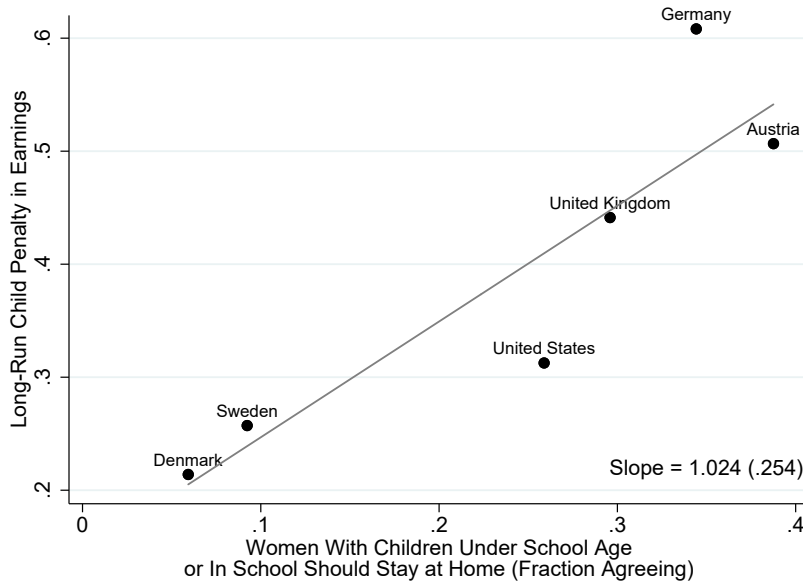
## 4 Child Penalties: Explanations

One set of explanations for the differences in child penalties focus on government policies. These include taxes, transfers, and family policies such as parental leave and child care provision that directly affect mothers' incentive to work. There is a voluminous literature on the impact of such policies on female labor supply and gender gaps (see [Olivetti & Petrongolo 2017](#) for a review). Of particular relevance, [Kleven \*et al.\* \(2019\)](#) consider the impacts of parental leave and public child care on the dynamics of child penalties. Their setting is Austria, a country where the combination of rich administrative data and a series of parental leave reforms and child care expansions allow for compelling quasi-experimental analyses of these questions.

They find that, in the long run, parental leave and child care policies have little or no effect on child penalties. They do find short-run effects of parental leave, however. Increasing the duration of paid and job-protected leave implies larger short-run child penalties in both earnings and employment. This suggests that some of the cross-country variation in short-run child penalties may be explained by variation in parental leave schemes, especially considering that the duration and

effects of subsequent children and therefore depend on total fertility. However, differential fertility is unlikely to drive the variation in child penalties across countries. For example, the German-speaking countries exhibit the largest penalties despite being characterized by the lowest realized fertility at event time 10. See Table [A.I](#) in the online appendix for descriptive statistics in each country.

Figure 4: Estimated Child Penalties vs Elicited Gender Norms



Notes: The figure plots our estimated long-run child penalties in earnings against elicited gender norms from the International Social Survey Program (ISSP). We focus on responses to an ISSP question of whether women with children under school age or in school should work outside the home (full-time or part-time) or stay at home. The figure plots child penalties against the fraction of respondents who agree that women should stay at home.

generosity of these schemes vary greatly across countries. For example, the larger short-run child penalty in Sweden relative to Denmark may be related to the longer and more generous parental leave offered in Sweden. Moreover, the small dip in the earnings of Swedish fathers following child birth could be explained by the presence of earmarked paternity leave in Sweden, as opposed to maternity leave or generic parental leave. In any case, despite these short-run effects, the main take-away from [Kleven \*et al.\* \(2019\)](#) is that child penalties are not driven primarily by public policies.

If policies cannot explain the large differences in long-run child penalties across countries, then what is the explanation? A natural candidate revolves around gender norms and culture, but it is hard to provide conclusive evidence on the importance of such mechanisms (see [Bertrand 2011](#) for a review and [Steinhauer 2018](#) for a recent application). In their study of Denmark, [Kleven \*et al.\* \(2018\)](#) show that child penalties are transmitted through generations, from parents to daughters (but not sons). That is, girls growing up in families with a more traditional division of labor between the parents incur larger child penalties when they themselves become mothers. These findings are consistent with an influence of the family environment in the formation of women's preferences

over family and career.

For the full set of countries studied here, Figure 4 provides evidence on the relationship between child penalties and elicited gender norms. The norm variable is taken from the International Social Survey Program (ISSP), focusing on a question of whether women with children under school age or in school should work outside the home (full-time or part-time) or stay at home. The figure plots our estimated long-run child penalties in earnings against the fraction of respondents who think women should stay at home. The correlation between child penalties and gender norms is quite striking. The countries that feature larger child penalties are also characterized by much more gender conservative views. This evidence, while not necessarily causal, is consistent with a potentially important role for gender norms.

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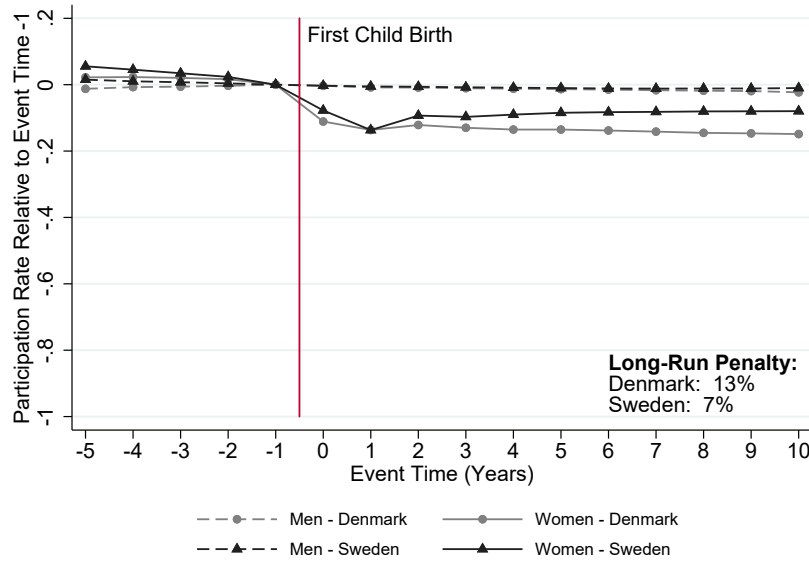
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# Online Appendix

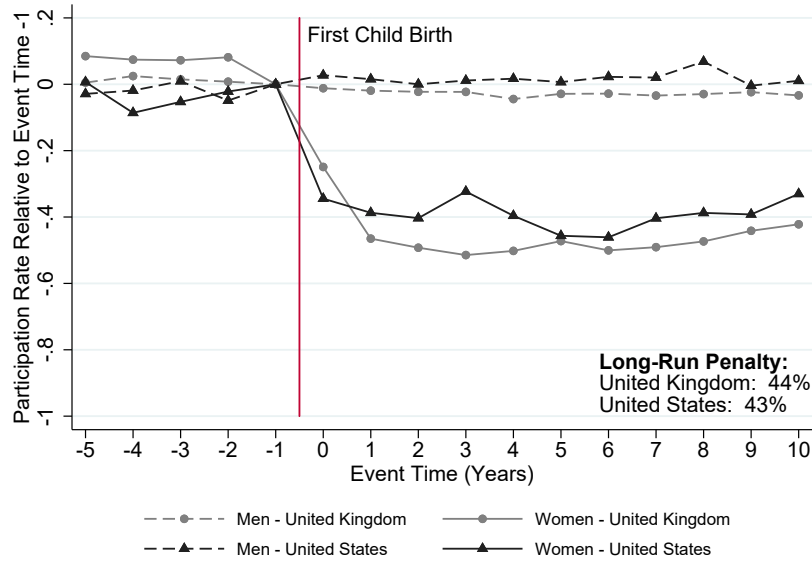
## A Supplementary Figures

Figure A.I: Child Penalties in Participation Rates in Scandinavian Countries



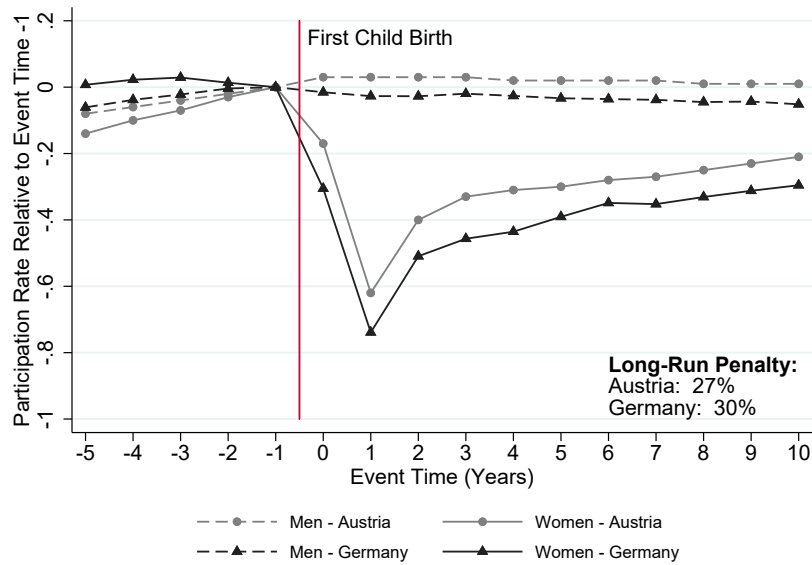
Notes: The figure shows percentage effects of parenthood on the participation rate across event time  $t$  for each gender  $g$ , i.e.  $P_t^g$  defined above. The figure also displays long-run child penalties, defined as the average penalty  $P_t$  from event time 5 to 10.

Figure A.II: Child Penalties in Participation Rates in English-Speaking Countries



Notes: The figure shows percentage effects of parenthood on the participation rate across event time  $t$  for each gender  $g$ , i.e.  $P_t^g$  defined above. The figure also displays long-run child penalties, defined as the average penalty  $P_t$  from event time 5 to 10.

Figure A.III: Child Penalties in Participation Rates in German-Speaking Countries



Notes: The figure shows percentage effects of parenthood on the participation rate across event time  $t$  for each gender  $g$ , i.e.  $P_t^g$  defined above. The figure also displays long-run child penalties, defined as the average penalty  $P_t$  from event time 5 to 10.

## B Data Description

The methodology applied to compute child penalties in this paper relies on [Kleven \*et al.\* \(2018\)](#). We estimate the impact of children on the labor market trajectories of mothers and fathers using event studies around the birth of the first child. This approach requires high-quality panel data with information on labor market outcomes and children.

In this section, we explain the data sources and sample selection criteria used for all 6 countries. In order to make the estimates as comparable as possible, we have followed the same sample selection principles in all the countries.

We consider individuals who have their first child between the ages of 20 and 45, and who are observed between 5 years before and 10 years after child birth. Our analysis focuses on first child births where the parents are known and alive. We do not impose any restriction on the relationship status of parents and include all individuals who have a child in a given year and can be followed over time whether or not they are married, cohabiting, separated, divorced, or have not yet formed a couple in any given year.

### B.1 Austria

#### B.1.1 Data Sources

The Austrian dataset consists in the Austrian Central Social Security Register (ASSD), which provides very detailed longitudinal information and covers the whole population of dependent employees between 1980 and 2017.

**Data Availability** The original ASSD data can be obtained from Statistics Austria. To that end, please contact [Statistics Austria information services](#).

#### B.1.2 Variable Definition

**Earnings** Yearly earnings are defined as the sum of the basic wage and supplementary payments such as bonuses, 13th and 14th monthly wages and extra vacation payments. Due to the fact that the lower tail of the earnings distribution is not observed in the original dataset, we are in fact

Table A.I: Descriptive Statistics

Country	Year of First Child		Age at First Child		Number of Children
	Range	Average	Men	Women	At $t = 10$
Austria	1985-2007	1995	30.0	26.5	1.7
Denmark	1985-2003	1994	28.5	26.2	2.2
Germany	1989-2005	1997	30.4	27.7	1.9
Sweden	1997-2011	2004	30.8	28.7	2.2
United Kingdom	1991-2008	1998	31.3	30.0	2.0
United States	1967-2006	1985	25.8	24.9	2.1

ignoring the earnings of minor employment. Note that earnings in the social security data are top coded at the top of the earnings distribution. Earnings are adjusted to take inflation into account.

**Employment** The ASSD records all employment spells for the universe of individuals. An individual is considered as working in a given year if there has been at least one employment spell in that year lasting one day or more.

**Birth Information** Child births for mothers are registered in the social security data, but fathers are not observed in this dataset. Fathers are registered alongside mothers in the dataset of child benefits and matched to our main dataset until 2013. Event time is defined as the difference between the year in which earnings and employment status are reported and the date of birth of the first child. Due to the fact that civil servants are included in the sample but only observed after 1988, we might potentially misattribute some births as the first child when in fact they correspond to the second or third child. We conducted a robustness check excluding all individuals ever observed in public employment, making up 4% of the sample, and the results remained unchanged.

### B.1.3 Sample

The sample contains all first child births where the parents are observed every year between 5 years before having a child and 10 years after. Descriptive statistics are reported in Table [A.I](#).

## B.2 Denmark

### B.2.1 Data Sources

The analysis of child penalties in Denmark relies on administrative data for the full Danish population between 1980 and 2013. We combine several administrative registers linked at the individual level via personal identification numbers to obtain a dataset of parents containing their labor supply, demographic information and the date of birth of their first child. Crucially, the data allows us to link family members.

**Data Availability** The data from our project can be requested from the [Statistics Denmark's Division of Research Services](#) (ask for a copy of our project #70629).

### B.2.2 Variable Definition

**Earnings** The Income Statistics Register contains information about all economically active individuals. Earnings are defined as labor income before taxes and transfers. They are observed at the calendar year level, i.e. as the sum of total gross earnings from January to December of each calendar year.

**Employment** To define our employment status variable, individuals with reported non-zero earnings are considered as working.

**Birth Information** Information on births stems from birth registry data and enables to link parents to their children. Event time is defined as the difference between the year relevant for earnings and the year of birth of the first child.

### B.2.3 Sample

Our analysis focuses on first child births where the parents are known, alive and reside in Denmark. The sample contains all first child births where the parents are observed every year between 5 years before having a child and 10 years after. Descriptive statistics are reported in Table [A.I](#).

## B.3 Germany

### B.3.1 Data Sources

Our German dataset is a representative longitudinal survey, the German Socio-Economic Panel (SOEP), which provides rich information on labor supply dynamics and allows to observe the date of birth of children to adult members of the sample. It was conducted between 1984 and 2016 in West Germany and between 1990 and 2016 in East Germany.

**Data Availability** The data can be requested from the [Research Data Center of the German SOEP](#).

### B.3.2 Variable Definition

**Earnings** When surveyed, individuals are asked to report the total of their gross labor income in the last 12 months. If an individual is surveyed in event year  $t$ , we therefore define their reported gross labor income in the survey as their earnings for event year  $t - 1$ .

**Employment** Employment status of the parent is defined based on wages and hours worked. More precisely, an individual is considered employed if he had positive wages and worked at least 52 hours in the previous year.

**Birth Information** Year of birth is reported for each child. Event time is defined as the difference between the survey year and the year of birth.

### B.3.3 Sample

Our analysis focuses on first child births for the parents observed in the SOEP. The sample contains all first child births where the parents are observed every year between 5 years before having a child and 10 years after. Descriptive statistics are reported in Table [A.I](#).

## B.4 Sweden

### B.4.1 Data Sources

The dataset for Sweden is a combination of fiscal statistics and registry data providing comprehensive information about the universe of births between 1997 and 2011.

**Data Availability** The data can be requested from [Statistics Sweden MONA \(Microdata Online Access\)](#) support.

### B.4.2 Variable Definition

**Earnings** Information about earnings and employment status stems from the longitudinal integration database for health insurance and labour market studies (LISA), which includes all individuals who are at least 16 years old and registered in Sweden. Earnings are defined as labor income before taxes and transfers. They are observed at the calendar year level, i.e. as the sum of total gross earnings from January to December of each calendar year.

**Employment** To define the employment status variable, all individuals with non-zero earnings are considered as working.

**Birth Information** The birth dates of first children are inferred from the Swedish birth registry. Event time is defined as the difference between the year for which earnings and employment status are reported and the year of birth of the first child.

### B.4.3 Sample

Our analysis focuses on first child births where the parents are known, alive and reside in Sweden. The sample contains all first child births where the parents are observed every year between 5 years before having a child and 10 years after. Descriptive statistics are reported in Table [A.I](#).



## B.5 United Kingdom

### B.5.1 Data Sources

Our analysis of child penalties in the United Kingdom is based on the British Household Panel Survey (BHPS), following households for 18 years between 1991 and 2009.

**Data Availability** The BHPS can be obtained by contacting the [UK data service](#).

### B.5.2 Variable Definition

**Earnings** Reported earnings are defined as annual labor income before taxes and transfers.

**Employment** Participation is defined as having positive earnings last year.

**Birth Information** To identify parents who have had children before the BHPS starts, we use on fertility history interviews, conducted in the second wave after entry into the sample. Unfortunately, this fertility history is not asked again after the second wave, so we use the household grid, which lists all individuals who live in a household together, relationships between household members, and some basic demographic information, such as date of birth. In order to calculate event time, we calculate the difference in months between the dates of interview and of first child, divide it by 12, and round it to the nearest integer. Event times greater (less) than  $t = 10$  ( $t = -5$ ) are capped  $t = 10$  ( $t = -5$ ).

### B.5.3 Sample

The sample focuses on first child births for the parents surveyed in the BHPS. It includes all individuals who have a child in a given year and can be followed over time. The restriction imposed on the Danish administrative dataset in [Kleven \*et al.\* \(2018\)](#) that parents need to be observed every year would be excessively stringent in the case of the survey-based data in the United Kingdom. In order to mitigate selection effects and to ensure that the sample is as close as possible to a balanced panel with respect to event time, our sample includes parents who are observed (1) At least once before (5 years before first child birth to the year of birth) and once after their first child birth

(one year after to 10 years after first child birth); (2) At least 8 times over the 15-year window. We further restrict age at first birth to be between 20 and 45. Descriptive statistics are reported in Table [A.I](#).

## **B.6 United States**

### **B.6.1 Data Sources**

The Panel Study of Income Dynamics (PSID) follows a nationally representative sample of the American population from 1968 to present. Our dataset includes household heads and their spouse or cohabitant, for which we observe labour market outcomes and family history consistently.

**Data Availability** The PSID data are available [online](#) and free of cost to researchers.

### **B.6.2 Variable Definition**

**Earnings** Given the objective of the PSID, information about earnings is precise and detailed. Earnings are defined as total labour income before taxes and transfers, including farm income, business income, wages, bonuses, overtime pay, commissions, as well as income from professional practice and roamers and boarders. They are reported by the respondents and their spouses or cohabitants for the year prior to the interview and are attributed to our earnings variable in year  $t - 1$ . For individuals who are not working, earnings are coded as 0 rather than as missing values. There is censoring of reported income at the very top of the distribution; however, the thresholds are high and the number of observations affected by top-coding is tiny.

**Employment** Our employment status variable is based on the answer to the question "Are you working now, unemployed, retired, or what?". We consider as working all the individuals who indicate being working at the time of the survey, temporarily laid off or on sick leave, whereas unemployed, retired, permanently disabled individuals, students and individuals keeping house are considered as not working.

**Birth Information** The PSID asks about the birth year and month of the individual's first or only child. Date of birth of the first child is known with different degrees of precision, up to the

month, the season or the year. In the two latter cases, several assumptions are made. First, we assume that children born in winter are born in January, and those born in the spring, summer or fall respectively in April, July and October. When first child birth is only known up to the year, we assume that it occurred in September. Event time is defined as the difference between the interview date and the birth date of the first child, rounded to the closest year. Interview date is available up to the fortnight from 1968 to 1979 and up to the day from 1980 to 2015, which allows to impute with relative precision the month of survey response.

### **B.6.3 Sample**

The sample focuses on first child births where the parents are known, alive and reside in the United States. No restriction on the relationship status of parents is imposed and the sample includes all individuals who have a child in a given year and can be followed over time. The restriction imposed on the Danish administrative dataset in [Kleven \*et al.\* \(2018\)](#) that parents need to be observed every year would be excessively stringent in the case of the survey-based data in the United States. In order to mitigate selection effects and to ensure that the sample is as close as possible to a balanced panel with respect to event time, our sample includes parents who are observed (1) At least once before (5 years before first child birth to the year of birth) and once after their first child birth (one year after to 10 years after first child birth); (2) At least 8 times over the 15-year window. We further restrict age at first birth to be between 20 and 45. Descriptive statistics are reported in Table [A.I.](#)

## **B.7 Data on Gender Norms**

### **B.7.1 Data sources**

Data on gender norms stem from the International Social Survey Programme (ISSP). The relevant survey years are discussed in the following sections.

**Data Availability** The ISSP data are partly available [online](#) and upon request.

Table A.II: Available ISSP Waves for Gender-Related Questions By Country

Country	ISSP Wave			
	1988	1994	2002	2012
Austria	Yes	Yes	Yes	Yes
Denmark	No	No	Yes	Yes
Germany	Yes	Yes	Yes	Yes
Sweden	No	Yes	Yes	Yes
United Kingdom	Yes	Yes	Yes	Yes
United States	Yes	Yes	Yes	Yes

### B.7.2 Data Treatment

Our study focuses on two survey questions: "Do you think that women should work outside the home full-time, part-time or not at all under the following circumstances?"

- "When there is a child under school age"
- "After the youngest child starts school"

For each country  $c$  and wave  $t$ , we define  $N_{ct}$  as the average fraction of individuals answering "should stay at home", across these two questions, weighting by survey weights. We then take for each country  $c$  the unweighted average of  $N_{ct}$  across all available waves. We consider all respondents aged between 16 and 64, regardless of their employment status and family situation. Moreover, the ISSP was conducted separately for West and East Germany, Northern Ireland and Great Britain. To compute the country-wide response rates, regions were weighted by their share in total population of the country. More precisely, we assumed that Northern Ireland and Great Britain make up 3% and 97% of population in the United Kingdom respectively, and West and East Germany 82% and 18% of total population in Germany respectively.